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Dear Gary:

In this letter I present my review of the manuscript titled **Testing the Taxonomic Validity of Preble's Meadow Jumping Mouse (*Zapus hudsonius preblei*)** by R. R. Ramey II, H.-P. Liu, and L. Carpenter, revised 12 March 2004.

I find this paper very clear in its presentation, use of hypothesis-testing, and overall good science. The evidence suggests that *Zapus hudsonius preblei* is not genetically or morphologically distinct from *Z. h. campestris*. I am not a geneticist, and will limit my subsequent comments to the other parts of the paper.

The morphometric data and analysis appear solid. Krutzsch (1954) acknowledged that *Z. h. preblei* most closely resembled *Z. h. campestris* and the techniques he used were the best available science for his time. Also, it was recently brought to my attention that a more recent study of the systematics and biology of the genus *Zapus* found insufficient morphological evidence to support subspecific status for *Z. h. preblei* (Jones 1981, as cited in Beauvais 1998).

While I agree that the qualitative descriptions can be vague and impossible to reproduce exactly, there is one character used by Krutzsch (1954) that may be straightforward to see and evaluate: The "incisive foramina long and usually truncated at posterior border" on *Z. h. campestris*, compared to "incisive foramina narrower, not truncate posteriorly" on *Z. h. preblei*. However, I consider this a moot point because the overall analysis clearly indicates a lack of morphological distinction between the two taxa.

The interpretation of reduced gene flow attributed to a southern colonization event seems quite plausible. And that hypothesis is preferred to the isolation of *Z. h. preblei* with a northward colonization and subsequent hybridization with *Z. h. campestris* because it is a more parsimonious explanation.

A southern colonization from the Black Hills to southeastern Wyoming could be very difficult through Thunder Basin due to dry conditions and the fact that the drainages run east-west, thus requiring the crossing of drainages and ridges and rendering movement difficult for a mouse. Perhaps a better avenue occurs along the north-south axis created by the Powder River on the east flank of the Bighorns, and the Belle Fourche, the drainage on which *Z. h. campestris* specimens were collected at Bear Lodge. Cooler, wetter conditions during two events in the Neoglaciacion period in the Rocky Mountains may have provided an opportunity for such movement along these drainages in the past 900 years or so. Most probably there are no trapping or collection data from these areas, a considerable gap in our knowledge. Interestingly, the indication that the four Albany County *Z. h. preblei* specimens were genetically *Z. princeps* removes some of the northernmost *Z. h. preblei* specimens from the picture.

I find the paper by Crandall et al. (2000) to be a clear treatment of evolutionary significant units (ESUs). I strongly agree for the need to incorporate ecological data, as well as morphometric data, along with genetic data. I find the broader categorization of population distinctiveness to be more realistic and appropriate than a dichotomous view because it is better able to address the complexity nature presents. In the paradigm presented by Crandall et al. (2000), *Z. h. preblei* (or *Z. h. preblei* and *Z. h. campestris* combined) may fit case # 6.

Although there appears to be a lack of readily-available published information, I find the important question of potential ecological differences between the two taxa, or between the combined *Z. h. preblei* and *Z. h. campestris* and the remaining subspecies, nonetheless unanswered. The Front Range likely has less moisture than the Black Hills area, and the combined Front Range/Black Hills area has less moisture than the more eastern range of the species. The potential ecological (and associated behavioral) uniqueness of *Z. h. preblei* in being restricted to riparian habitats is worthy of further investigation, and a better understanding of how *Z. h. campestris* fits into this ecological paradigm is of considerable interest. In the past 10 years or so, the recognition of the importance of animal behavior to conservation biology has grown (e.g., Caro 1998, Festa-Bianchet and Apollonio 2003).

I recognize that, following the guidelines of Crandall et al. (2000), ecological exchangeability should be demonstrably heritable. This may be difficult to show for the potential habitat differences described above. Furthermore, Crandall et al. (2000) indicate that in their review of 98 studies, ecological data were frequently lacking.

I think the status of *Z. h. campestris* now becomes an important biological question, as very little is known about this subspecies. This taxon is categorized as vulnerable by the International Union on the Conservation of Nature. Thus although *Z. h. preblei* may not be distinct, there is the possibility that the two subspecies together may be imperiled.

Conversations with Gary Beauvais of the Wyoming Natural Diversity Database were of benefit to me in developing a better understanding of certain elements of the Wyoming

landscape and for the Jones (1981) reference.

Please let me know if you have any questions.

References

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